Extract from a Letter from the Rev. F. Howlett, to W. T. Lynn, dated 1877, *November* 3.

By this post I forward you careful drawings of the fine solar spot (now on the disk) for October 31 and November 1, taken by projection, power 120, and screen 5 ft. 2 in. from eye-piece, when each half-inch subtends just 15″. The main spot must have been very near the solar equator, especially when we consider that the Sun's north pole is now within the margin of the disk.

The lesser group "followed" the principal one after an interval of about 4′ 30″.

I would add that a friend who was watching me as I took the drawings noticed, as well as did I, a very distinct *haze* of a kind of bistre-brown colour extending for some 10″ or 12″ upon the penumbra, beyond the margin of the umbra, on October 31, as well also as a considerable sized nucleus of about 12″ diameter within the umbra.

The definition on October 31 was very fine, and it was very good also on November 1, though I could barely distinguish any of the bistre-brown haze or the nucleus on the latter day.

The "bridge," extending more than half-way across the umbra, was not very luminous on October 31, and still less so on November 1, and on each day aforesaid terminated in very faint flocculent matter.

To-day (November 3), this flocculent matter is very much feebler still, and is all that remains of the "bridge."

Note on a Special Case of "the Most Probable Result" of a Number of Observations. By J. M. Wilson, Esq., Rugby.

It is beyond the reach of all but pretty good mathematicians to follow the reasoning about "probable errors" and "the most probable results" of a number of discrepant observations. But there is one law of weighting observations which lends itself so easily to calculation that perhaps some of the non-mathematical readers of the *Notices* may be interested in seeing it. The result is not new, though it was quite new to me when I first observed it, some six or seven years ago, and I do not remember to have seen it since, except in Mr. J. W. L. Glaisher's paper in the *Memoirs* for 1872.

A number of numerical readings are made of some magnitude that is to be measured; they differ from one another. What is the most probable result? The common method is to take their arithmetical mean; and if the readings are tolerably evenly distributed this will not give a bad result. But if one reading is widely different from the others, the doubt occurs whether it ought to be wholly rejected or have less weight assigned to it.

On the principle of the arithmetical mean there is no choice; the observation must be accepted wholly or rejected wholly, unless of course arbitrary weights are assigned to the observations by an *ex post facto* judgment of them.

But suppose we agree that observations shall have less weight the remoter they are from the final result; in fact that the weight of an observation shall vary inversely as its error from the result; so that, if our final result is 27·4, an observation 27·5 shall have more weight than an observation 27·9 in the ratio of 5 to 1, and more than an observation 28·4 in the ratio of 10 to 1.

The most probable result of a number of readings will, according to this law of weighting observations, be obtained as follows:—

Let the observations be arranged in order of magnitude; *then, if the number of them is odd the middle observation must be taken; and, if the number is even, anywhere between the two middle observations must be taken, supposing all to be unequal.*

For the case is this. Let a rod without weight have a number of particles resting on it having weights each inversely proportional to their distances from the point of suspension. Find the point on which it will balance.

If the distance of any particle from the point of suspension is x and its weight $\frac{1}{x}$, then its moment is 1, and all that is necessary is that there shall be an equal number of particles on both sides of the fulcrum.

Hence if a number of readings of the distance, we will suppose of a double star, are taken as follows: 27·3, 29, 28·3, 26, 26·8, 27·1, 28, 28·5, 27·4, they must first be arranged in order of magnitude, 29, 28·5, 28·3, 28, 27·4, 27.3, 27·1, 26·8, 26; and, the number of them being odd, the middle one, 27·4, is the most probable result. The arithmetical mean is 27·6.

If the last observation were cancelled, the most probable result would be anywhere between 28 and 27·4; the arithmetical mean is 27·8.

There is one curious case in which this method gives apparently *no* most probable result. This case would have arisen if 27·3 is substituted for 27·4. The most probable result cannot be 27·3, for there then would be three readings on one side of it and four on the other; nor can it be more than 27·3, for then there would be five readings on one side and four on the other. Of course the solution of the paradox is to be found by supposing the two equal readings to differ by an infinitesimal amount, and the real result would therefore be 27·3.

Rugby, Nov. 1877.